

of exactness of 1" annually. The presumption seems to me fairly strong that this accord has existed for the last 3000 years.

7. In view of the fact that, with the partial exception already mentioned, no ancient chronicler seems ever to have attributed the slightest importance to the question whether an eclipse of the Sun was or was not total at a given place, results based on the assumption that eclipses were total at the several places of record cannot be entitled to any great weight.

8. The act of rejecting the results of gravitational theory in order to secure the best possible representation of these supposed eclipses is subject to judgments similar to those pronounced by the Congregation of Cardinals against the doctrines of Galileo.

Reply to Professor Newcomb's Note. By P. H. Cowell.

The paragraphs are numbered so as to refer to the corresponding paragraphs of Professor Newcomb's note.

1. I agree with Professor Newcomb, but I would add that *à posteriori* evidence (the agreement of the equations of condition) has some value.

2. In all eclipses used by me, with the single exception of the eclipse of -762, there are words in the record implying at least a near approach to totality.

-1062	fire in the midst of heaven
- 647	day turned into night
-430 and - 309	stars seen
+ 197	light nearly extinguished

In giving first place to the eclipse of Xerxes, Professor Newcomb considers the words of the record only: he does not consider the interval of time and place between the event and the historian, nor the character of the historian for accuracy, as inferred from his other writings.

3. My [papers are not concerned with the question as to whether the eclipse, stated at the end of the first century A.D. to have been once upon a time total at the Hellespont, was the same as the eclipse of Agathocles.

4. The assumptions mentioned form a good working hypothesis. Only two eclipses would have been necessary if totality at a given place were certain. The concurrence of four additional eclipses is offered to make up for the uncertainty of the historical data.

5. Professor Newcomb is not confronted with this dilemma. Were it so, I should admit that the second horn must be

avoided at all costs. The choice lies between the first horn, as Professor Newcomb states it ; or

An acceleration of the Earth's orbital motion must be accepted which will make the eclipses agree well with the most probable interpretation of the records.

6. I am basing my hypothesis of a secular acceleration for the Sun upon the acceptance, and not upon the rejection, of the theoretical position of the node.

7. There are contemporary records of very few eclipses. Great numbers of partial eclipses must have occurred. I attach great weight to the fact that a very simple assumption makes the six eclipses considered central at specified places. In my view we have heard of those eclipses, because they were striking phenomena worth recording. The lunar eclipses and the transits of *Mercury* are not very searching tests, but so far as they go they confirm the assumption referred to.

8. I have not rejected the results of gravitational theory. I at once acquiesced in the following argument as soon as it was presented to me : *

i. The motion of the node accords with theory now to well within $20''$ per century, which is therefore the extreme value to be assigned to the action of unknown causes.

ii. Therefore the position of the node twenty-five centuries ago must be considered known to within $500''$.

I presume Professor Newcomb does not expect me to consider such an argument as the following conclusive :—

i. No theoretical reason is known for a change in the Earth's mean motion.

ii. Therefore the Earth's mean motion must be constant.

To expect this would be equivalent to saying that theory must always precede observation.

Finally I would say that the question is one of evidence. There must be a degree of evidence that would be considered sufficient to establish the fact. I have produced a considerable amount of evidence, all pointing one way ; and the conclusion seems to me highly probable.

On the Transits of Mercury, 1677–1881. By P. H. Cowell.

In this paper I examine the transits of *Mercury* to determine whether they support or contradict the supposition of a secular acceleration of $4''$ in the Earth's orbital motion that I have deduced from ancient solar eclipses.

It is well known that the secular acceleration of the Moon cannot be determined from modern observations on account of the term of long period, on which theory is altogether silent ;

* In my first paper on ancient solar eclipses I had overlooked Professor Brown's paper on the secular accelerations in the *Monthly Notices* of 1897.